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THE TOPOGRAPHIC MAPS OF THE UNITED STATES GEOLOGICAL SURVEY.

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OUR national Geological Survey was charged by Congress with the preparation of a geological map of the United States. As no suitable topographic map existed to serve as the base of geological coloring, the Director of the Survey proceeded to develop a topographic corps for the purpose of producing a map that should serve his needs. This action has from time to time called forth discussion; as, for example, recently in *The American Geologist.* Information on the subject from the standpoint of the several scientific governmental bureaus may be found in the "Testimony before the Joint Commission of Congress," in 1885. It is not my intention to discuss this subject here, as I have been more interested in the examination of the great geographical product of the Survey, than with the association of geographical work with one bureau of the government or another. The recent appearance of the first folio of the "Geologic Atlas of the United States," marking the beginning of a most important national publication, without parallel in magnitude in other countries, makes some account of the topographic maps of the Survey appropriate at the present time.

The first season of topographic field-work under the re organized survey was in 1882. Previous surveys had done much work in the West; but their styles of publication were discordant. Belts of litoral country had been mapped by the Coast Survey; similar belts along the Great Lakes had been mapped by the Lake Survey; and extensive maps were in progress under the Mississippi and Missouri River commissions. The areas covered by these various surveys and by the topographic division of the Geological Survey are indicated in the annual reports of the Director. No duplication of earlier work of acceptable character has been attempted; but the results gained by all the surveys are reduced to a uniform style of publication, on a plan that will in time embrace the entire country.

Details of the plan adopted for the topographic survey are presented in the annual report of the Director for 1884–85. Our country includes an area of 3,000,000 square miles; hence the need of economy. The elaborate methods applicable in smaller European nations could not be introduced. An astronomical and computing division took charge of the determination of fundamental points, where needed. A triangulation division extended the determination of points from those furnished by the astronomical parties or from previous work by the Coast or other Surveys. The Appalachian triangulation, for example, is illustrated in the sixth annual report of the Survey. While this triangulation suffices for the needs of the proposed maps, it is not of such accuracy as would be required in determining the figure of the earth; being in this respect different from the fundamental triangulations of our Coast Survey and of most foreign countries. The saving of time and money, demanded in the survey of so great an area, required this modification of former methods. Numerous topographical parties were placed in the field to fill in the spaces between the smaller triangles. The plane-table was employed in many districts; traverse surveying was adopted elsewhere. Railroad levels have been extensively utilized. Much sketching between established points has been required; and to any one who carries the published maps into the field, it will be apparent that the details of topographic form are generalized, and are occasionally incorrectly represented. on account of the rapidity with which the work was carried on. It was difficult at first, as it may be still, to secure the services of a sufficient number of experienced topographers; but their practice in the field has added to their expertness in sketching streams, ridges, and contours. Much effort has been given to their improvement; practical field instruction has been given to the newer men during the summer months, and voluntary meetings for discussion of methods have been held during the winter in Washington.

Those who wish to examine the style of work followed in the preparation of various sample sheets, should consult the eighth annual report, in which Mr. Gannett has included several graphic illustrations of the number and distribution of points determined by intersections, or of lines run by traverse. Yet it is manifest that much is left to sketching, and, judging from the amount of country covered by a single party in a season's work, the sketching must often be rapid.

In common with many others, I wish that the accuracy of the surveys might have been greater; but I presume that all questions of method, scale, style, cost, and equitable distribution of work were duly considered by Major Powell, Director, Mr. Gannett, Geographer, and other members of the Survey, and decided as seemed best in view of all controlling circumstances. Not least among these controls is the disposition of Congress to support the work. Judging by the appropriations recently voted, it would take the greater part of a century to survey the country by the more elaborate European methods. Our grandchildren and not ourselves would enjoy the products of the work, and few of us are so unselfish as to be satisfied with so long a postponement.

As to publication, three scales were at first adopted; 1:62,500; 1:125,000; and 1:250,000; corresponding to about one, two and four miles to the inch. The map-sheets were divided according to lines of latitude and longitude, a sheet on the smallest scale covering a "square degree," while four and sixteen sheets of the larger scales are required for the same area. Each sheet is named after its chief town and State; and on the later sheets, the name of the four sheets adjoining it are printed on its sides: the four corner-wise sheets might be to advantage indicated in the same way. According to the scale of the map and the complication of the topography, contours are drawn with vertical intervals varying from 10 to 200 feet. Each sheet is printed in three colors, as described in the sixth annual report: black for the artificial features and names, all lettering following a consistent plan; blue for the hydrography, brown for the contours. All the sheets thus far issued are printed very clearly, and as a rule accurately; the earlier ones by Bien of New York, the latter ones by the Survey itself.

According to information lately received from Mr. Gannett, the number of sheets completed up to June 30, 1892, is as follows: 295 on scale of 1:62,500; 338 on 1:125,000; and 61 on 1:250,000; making a total of 694. The total area surveyed, but not all engraved, to that date was 547,000 square miles, distributed over forty-two States and Territories. In future, only the inch-to-a-mile scale will be employed, and the surveys of the current year are conducted with regard to this new feature of the plan.

Before speaking of certain sheets of the contoured maps, mention should be made of several smaller scale maps of the country as a whole. The most important of these is the nine-sheet map, on a scale of 1:2,500,000, with contours for 100, 500, 1000, 1500, 2000, and higher thousands of feet.

A limited edition of this map has been prepared for office use without the black print of artificial features and names, thus giving desirable prominence to the natural features. This is an

extremely valuable map, as it contains all available information concerning the relief of our country, expressed in a form most generally useful. A smaller sheet, including the whole country, represents increasing altitude by a series of nine brown tints; a very effective presentation destined to serve as the original of many a map in our school geographies.

A visit to the office of the Survey in Washington discloses the great fund of information there gathered on the topographic map-sheets. Unhappily the sheets are not at present published for general distribution; they are chiefly for the use of the members of the Survey in their geological studies. The edition of each sheet as printed is stored in a room containing a great number of shelves, each one marked in front with the name of its map. Hardly a sheet can be drawn from its shelf without revealing some interesting features to the geographer, the geologist, or the engineer.

It is only of the first of these interests that I shall speak. Although when examined on the ground that they represent, the maps are not so accurate in detail as we might wish, still the graphic view of the country that they present is so vastly more minute than any to which we have hitherto had access that it is something of a revelation to look over them. A great fund of geographical information is stored up in that map-room. One may look forward to an auspicious geographical day when the maps are generally distributed, and when that universal study, home geography, is enriched by the illustrations that they will afford. Some of the smaller eastern States have already reached this happy position, by means of two-party arrangements between themselves and the national Survey. The first of these was New Jersey. After the Coast Survey had furnished the triangulation, and after about half of the mapping had been borne by the State, the Geological Survey sustained the remainder of the cost of field-work; but in this State, the form of publication is somewhat different from that adopted for the rest of the country. The sheets are larger, and overlap to a considerable extent, so that there is no place that is not well within the boundary of one sheet or another. The publication of the seventeen inch-to-a-mile sheets and of the shaded relief map of the State on a scale of five miles to an inch, has been duly noted in *Science* at the time of their appearance. The separate sheets may be obtained from the State Geological Survey for 25 cents a piece, or $5 for the complete atlas of twenty sheets. Massachusetts was the next State to take advantage of the two-party arrangement. The triangulation here was taken chiefly from the old Borden survey. Now the whole State is covered in 54 inch-to-a-mile sheets; and a four-sheet map, on a scale of four miles to an inch with contours every hundred feet, has been published. Like the New Jersey atlas, the 54 sheets of Massachusetts may be bought of the Commissioners (address, Commonwealth Building, Boston) for six dollars. Composites of the sheets about Boston and Worcester have been published by the Appalachian Mountain Club for use in field excursions and otherwise. Rhode Island is next to be mentioned. Here the area is so small that the several sheets covering the State, with parts of Massachusetts and Connecticut next adjoining, have been mounted on rollers, and through the active interest of the State Commissioners, the local Legislature has been induced to make a special appropriation of $3,500 for the distribution of the mounted map to all public schools and libraries within the State,—a wise and liberal step towards better public education. The mounted map is also sold by J. C. Thompson, Providence, at $2 a copy. It is manifest that large States may follow this plan, by mounting grouped sheets in roller maps about four by five feet; so that every school should be provided with a large map of its own part of the State. If one may judge by the small appreciation that teachers generally have of the physical features of their own regions, such a distribution of maps is greatly needed.

In other parts of the country, there are no States completely mapped as yet. A large area of country has been covered along the central and southern Appalachians, and the first folio of the Geologic Atlas was a map from this district, with others soon to follow. Missouri and Kansas have the good fortune to be represented by a large number of contiguous sheets; and in the western States and Territories the maps of the older surveys have been redrawn and printed on the new uniform plan. The various lines of interest suggested by these maps would lead me to write many pages, were they all followed; and I shall therefore limit myself here to the one which takes my first attention, the physical features of our home geography. This may be illustrated by a brief reference to five maps from Missouri,—the Tuscumbia, Versailles, Warsaw, Clinton, and Butler sheets.

These sheets run from east to west, partly across the central part of the State, somewhat south of the Missouri River, and include the greater part of the basin of the Osage River. The eastern course of the Osage, towards its mouth in the Missouri, is seen to be extremely tortuous in a steep-sided valley, trenched two or three hundred feet below the level of the surrounding upland. The meanders of the river are peculiar in not being, like those of the Mississippi, spread upon a flat flood-plain. High spurs of the upland occupy the neck of land between every turn of the stream. Evidently, the meanders are not of the ordinary type. It has been suggested that they result from the jointed structure of the rocks that the river traverses; but this is hardly possible, for it would not explain the manifest relation that exists between the size of the river and the radius of its swinging curves: the larger the stream, the larger the radius. I have therefore supposed that we have in this curiously curved valley an illustration of a process long recognized in the case of various rivers of northern France. Briefly stated, this is as follows: Once upon a time, a river, long active, had worn down its basin to a surface of faint relief. Its valley sides had wasted away so as to oppose little interference to its lateral swinging. Its slope had become very gentle, and its current had taken to a deviating path, peculiar to old streams, which so generally meander on their flat flood-plains. Then the region that it traversed was evenly raised to a greater altitude, and the faithful stream once more turned to the task of cutting down its channel close to base-level and carrying away all the waste that was fed into it. But in doing so, it retained in the new cycle of its life the meandering course that it had attained in its old age in the previous cycle. Although its activities were rejuvenated, its habit of swinging from side to side was still preserved. It behaves as if it were on a flood-plain, although the flood-plain, on which it learned this behavior, has been consumed. The Seine and Meuse have extremely meandering channels in deep and rather steep-sided valleys; and I have learned from my most obliging and well-informed correspondent, Mr. E. de Margerie, that the above explanation is current regarding them. The Mosel also has a deep meandering gorge between the Eifel and the Hunsrück, in western Germany. In this country, I have supposed that the meanders of the north branch of the Susquehanna, in the plateau of northeastern Pennsylvania, might be thus explained; and the incredible turns of the unpronounceable Connedogwinit, opposite Harrisburg, seem to be of the same kind, except that the Susquehanna learned its swinging habit on a Cretaceous lowland flood-plain, while the Connedogwinit was taught in late Tertiary times. On looking over the Missouri maps, I concluded that the Osage was another example of the same inherited habit; and in this case there is a neat little bit of confirmation on the western of the map-sheets named above that deserves mention.

If it is true that the curved course of the Osage is inherited from a flood-plain whose level lay across the top of the present valley when the land lay lower, we must suppose that, after gaining elevation to the present altitude, and thus gaining permission to cut down towards a new base-level, advantage would be taken of this permission first in the lower part of the river, and that the deepening of the channel would gradually work backwards up stream. Good fortune brought us upon the river while it is, as we may say, just in the act of thus adjusting its valley to the new altitude of its drainage area. We see the lower course already deepened, while the upper course still preserves its part of the flood-plain from which the curves of the lower river were inherited. The upper branches of the Osage flow upon broad flood-plains meandering freely, skirted by back-swamps, and frequently cutting off their curves and leaving ox-bow lakes to one side of their newer course. These upper branches preserve

what the lower part of the river has already lost: they tell us what it has been, while it foretells what they shall come to be.

Of course, while only the maps are before me, and the Osage is a long thousand miles away, I do not wish to assert that this sketch of its history is demonstrably true; although I am strongly persuaded that an examination of the region on the ground would discover evidence confirmatory of it. The upland is built of nearly horizontal Paleozoic rocks. If they had stood at their present height above the sea ever since the date of their deposition, they would now be worn down close to sea-level, without retaining any distinct relief. Their narrow valleys show that this supposition is out of the question. The rolling upland in which the narrow valleys are incised is itself a surface of denudation; and as its reliefs are faint, with long gentle slopes and broad open valleys, beneath whose floor the narrow deeper valleys are incised, I am driven to the belief that the upland was for a long time a lowland, and that its gentle eminences are merely the remnants of a once higher mass. The dates at which this older denudation was carried on, and the later date at which the uplift to its new altitude was given, are not well determined; although from analogy with more eastern parts of the country, where the dates of such changes have been better made out, I am inclined to say that the Missouri upland was a lowland well into Tertiary time; and that the new trenches of the Osage and its neighbors were begun in consequence of an uplift somewhere about the close of Tertiary time.

These are suggestions rather than conclusions; but they still serve to illustrate the incentive to geographical study that the topographic maps supply. We all knew that there was a fertile field for study in our home geography; every one in his own district enjoyed cultivating his patch of the field; but now through the publication of these maps, it is as if the whole field was opening to all of us; and a rich geographical product is promised to all who enter it.

SUN-HEAT AND ORBITAL ECCENTRICITY.

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THE reader of Sir Robert Ball's important work, "The Cause of an Ice Age," needs no reminder that its argument rests upon a foundation of theoretical astronomy. To secure the essentials of the discussion one must read between the lines. It is the object of the present paper to select and arrange a few of the more simple inter-linear readings, in the hope that they may be serviceable in that borderland where astronomy, geology, and meteorology have each a claim.

1. "There can be no doubt that when the eccentricity is at its highest point the earth is, on the whole, rather nearer the sun, because, while the major axis of the ellipse is unaltered, the minor axis is least." ("The Cause of an Ice Age," p. 79). This is equivalent to saying that the mean distance of the earth from the sun is a function of the eccentricity of the earth's orbit, and is, moreover, such a function that when the eccentricity is a maximum the function is a minimum. The mean or average length of the radius-vector of an ellipse depends on the law assumed in regard to its variation. From the standpoint of geometry, disregarding kinematical and dynamical considerations, the simplest assumption is, that the vectorial angle is the fundamental variable. If the equation to the ellipse be written

$$r = \frac{a(1-e^2)}{1+e\cos\theta}$$

and r' be the mean length of the radius-vector, we may easily show that

$$r' = \frac{1}{\pi}\int_0^\pi r\,d\theta = a\sqrt{1-e^2}. \quad (1)$$

But in any investigation dealing with the amount of light or heat received by the earth a different assumption should be made; for it is clear that if the earth moves most slowly when in aphelion the effect is the same as if it were, on the whole, farther away from the sun. Assuming that the time is the fundamental variable and that the radius-vector sweeps over equal areas in equal times, we may find the average of the radii-vectores corresponding to the successive equal time-intervals. Consider a point moving in a circle whose centre is one focus of the ellipse. Let its areal velocity be equal to that of the point describing the ellipse, and suppose that when the radius-vector of the ellipse has swept through 180°, the radius, r_0, of the circle has swept through the same angle. Then

$$r_0^2\frac{d\theta_0}{dt} = r^2\frac{d\theta}{dt} = 2c = \frac{\pi a^2\sqrt{1-e^2}}{T},$$

where $2T$ is the periodic time. Integrating between the limits 0° and 180°

$$\pi r_0^2 = \pi a^2\sqrt{1-e^2}, \quad \text{or} \quad r_0 = a\sqrt[4]{1-e^2}. \quad (2)$$

r_0 is thus a minimum when e is a maximum, and *vice versa*. The value r_0 in (2) is greater than the value r' in (1), as we might have known in advance by simply comparing the two assumptions respecting the law of variation of r.

Developing the factor

$$\sqrt[4]{1-e^2}, \quad r_0 = a\left(1 - \frac{e^2}{4} - \frac{3e^4}{32} - \ldots\ldots\right).$$

The present eccentricity of the earth's orbit is 0.01678. According to Leverrier it cannot exceed 0.077747. To take $r_0 = a$, the average of $a(1+e)$ and $a(1-e)$, that is, of the aphelion and perihelion distances, is therefore a close approximation to the mean value obtained with the assumptions above made. Laplace, in stating Kepler's third law, says, "The squares of their times of revolution are as the cubes of the transverse axes of their ellipses." (*Méc. Cél.*, II., 1., § 3). He uses the term "mean distance" in speaking of the satellites of Jupiter and Saturn, but not in such a way as to indicate that he meant the semi-major axis. Gauss, in his first mention of the semi-major axis, says, "Hinc semi-axis major, qui etiam distantia media vocatur, fit $= \frac{p}{1-ee}$". ("Theoria Motus," p. 4). Similarly, Sir John Herschel uses the terms "mean distance" and "semi-major axis" as interchangeable.

2. "The total quantity of heat which the earth receives during each complete revolution will be inversely proportional to the minor-axis of the ellipse." (p. 79). Let dh be the heat-increment received in the time dt, and μ the rate of variation of heat at a unit's distance. Then, since the quantity of heat received varies directly as the time and follows the law of the inverse square,

$$dh = \mu\frac{dt}{r^2}.$$

But from Kepler's second law,

$$r^2\frac{d\theta}{dt} = 2c, \quad \text{or} \quad \frac{r^2}{dt} = \frac{2c}{d\theta}. \quad \text{Hence}$$

$$h = \int_0^\pi \frac{\mu\,d\theta}{2c} = \frac{\mu\pi}{2c} \quad (3)$$

From this it appears that the quantity of heat received in passing from one end of the major-axis around to the other varies inversely as the areal velocity. But

$$2c = \frac{\pi a^2\sqrt{1-e^2}}{T},$$

and since the length of the year is constant and the major-axis is constant, the areal velocity is to be viewed as a function of e alone. Suppose e becomes e' and let c' denote the new value of the areal velocity. Then $h' = \frac{\mu\pi}{2c'}$, and therefore $h : h' :: c' : c$. But $c : c' :: b : b'$; hence $h : h' :: b' : b$. Again, if we substitute $\frac{\pi a b}{T}$ for $2c$ in (3),

$$h = \frac{\mu\pi T}{\pi a b} = \frac{\mu T}{a^2\sqrt{1-e^2}} \quad (4)$$

Hence the amount of heat received in one year is the same that would be received if the earth were to move for a year in a circle whose radius is $a\sqrt[4]{1-e^2}$.

This accords with the result (2) already found for the mean distance of the earth from the sun. In a paper on the "Intensity of the Sun's Heat and Light" (Smithsonian Contributions to Knowledge, IX.), L. W. Meech calls $\frac{2\pi}{a^2 n \sqrt{1-e^2}}$ "the sum of the intensities during a complete revolution." In this expression n is the mean daily motion and equals $\frac{\pi}{T}$. Substituting $\frac{\pi}{n}$ for T and making μ equal to 1 in (4), the latter reduces to Meech's formula.

3. "If any two chords of the earth's orbit, as AX and BY, be drawn through the sun, S, the amount of heat received in passing over the arc AB equals the amount received in passing over XY." (p. 82). Samuel Haughton ("New Researches on Sun-Heat," 1881) proves by another simple application of Kepler's second law that the quantity of heat received by the earth in a given time is proportional to the angle described in that time by the radius-vector. For

$$r^2\, d\theta = 2c\, dt,$$

$$d\theta = \text{increment of true anomaly},$$

$$\frac{dt}{r^2} = \frac{d\theta}{2c} = \text{heat in the time } dt.$$

This is but a mathematical translation of the argument given by Herschel in "Outlines of Astronomy," 5th ed., § 368 b. The statement made on page 82, "Cause of an Ice Age," is verified by an employment of Haughton's expression. For since

$$dh \propto \frac{dt}{r^2}, \quad dh \propto \frac{d\theta}{2c}; \text{ hence}$$

$$h \propto \frac{\theta_2 - \theta_1}{2c}. \text{ Now } ASB = XSY = \theta_2 - \theta_1,$$

and the proposition is established. The law that "the amount of heat received in any given interval is exactly proportional to the true anomaly described in that interval" appears to have been first published by Lambert in his "Pyrometrie," 1779.

4. "The total heat received by the earth from equinox to equinox is equal to that received while completing its journey around the remaining part," (p. 88). The preceding demonstration does not involve the inclination of the chords to each other, neither does it involve the direction of either chord. Hence we may make X coincide with B and Y with A, and let the one resulting chord be the line of equinoxes, and the proposition follows.

5. "If δ be the sun's declination the amounts of heat received by the Northern Hemisphere and the Southern are to each other as $1 + \sin\delta$ to $1 - \sin\delta$." (p. 175). Draw a circle representing a section through the centre of the earth (regarded as a sphere). Let the horizontal diameter produced represent the celestial equator projected in a right line EE'. Through the centre of the circle draw AA', making an angle δ with EE'. AA' will be the axis of the cylinder of heat-rays falling upon the earth when the sun's declination is δ. Draw a diameter, DD', perpendicular to AA', and at the upper extremity of DD' draw an element, TT', of the cylinder. To this draw a parallel, CC', intersecting EE' at the circumference of the circle. TT' and CC' evidently include the portion of the cylinder falling on the Northern Hemisphere. If $2R$ is the length of the diameter, the perpendicular distance between TT' and CC' is seen to be $R + R\sin\delta$. Hence if $\frac{2H}{r_0^2}$ be the quantity of sun-heat falling perpendicularly on an area equal to the section of the earth at the mean distance r_0 from the sun in the unit of time, $\left(\frac{R + R\sin\delta}{2R}\right)\frac{2H}{r_0^2}$ is the part falling on the Northern Hemisphere, while the remainder, $\left(\frac{R - R\sin\delta}{2R}\right)\frac{2H}{r_0^2}$, falls on the Southern Hemisphere. These amounts are to each other as $1 + \sin\delta$ to $1 - \sin\delta$.

One or two other propositions will be discussed in a subsequent article.

ON A PHYSIOLOGICAL CLASSIFICATION OF THE OPHIDIA — WITH SPECIAL REFERENCE TO THE CONSTRICTIVE HABIT.

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THE writer would be the last to suggest a classification of any group of animals whatsoever based upon physiological data alone. Function, unless correlated with definite variation of structure, is never to be depended upon as a means of establishing specific differences. In illustration of this, one has only to cite the numerous examples of change of function, not simply within historic times, but even within the memory of living man, owing to variation in the environment of the creatures themselves. Witness the Kea, or New Zealand parrot, and the baboon of South Africa, both of which have become carnivorous since the introduction of sheep into this region; the bees of England, which, in certain districts, have within the last twenty years become frugivorous; and certain colonies of bats, inhabiting the islands of the Gulf of Paria in Trinidad, which have of late years taken to fishing, and have in consequence abandoned their nocturnal habits, and are now strictly diurnal beasts of prey. It is true that in certain isolated cases a change of function is followed by very slight variation of physical structure. In that of the domestic cat the intestine has certainly become elongated, and has probably undergone a further process of elongation in consequence of its less purely carnivorous diet; in particular, the duodenum has become more extended within recent centuries, if one may judge from analogy when comparing the creature with its wild prototypes.

In the case, however, of serpents, the family resolves itself into three groups so naturally in accordance with the manner in which they take their food, as to suggest the justification of a natural grouping founded on this basis.

If we had a specimen of every kind of snake before us, and could watch them in the act of feeding, we should see that they perform this process in three different manners. The majority, numbering probably 1,000 or 1,200 out of the 1,800 known species, simply catch the creatures on which they prey by the prehension of their jaws and long curved teeth, and work them gradually into the gullet on what we may call general principles.

A great disproportion exists between the size of the captor and of the captive. If the serpent be very much larger than the animal which it swallows, the latter is probably engulfed alive; but if, as is commonly the case, the captive is of large diameter proportionately to the œsophagus of the serpent, it is suffocated or crushed to death in the act of swallowing. As may be expected, the serpents that feed in this manner are such as live on what may be termed soft food, — frogs, lizards, fish, or other snakes.

But with the remainder we find two special provisions for the slaughter of the prey previous to deglutition — provisions so remarkable as to place the possessors in an entirely different category to the preceding. In one of these, and by far the smaller of the two subdivisions, numbering probably not more than 220 species altogether, or about one-eighth of the whole number of snakes, we find the death of the prey is encompassed by the injection of a morbific fluid, the venom. That this in the majority of cases serves as ammunition for the destruction of the captive cannot be doubted; but whether this is the primary reason why these creatures are gifted with venom is not so certain, seeing that in many species it probably comes very little into play for this purpose — e. g., in the sea snakes, in which the fangs are so short that the fish on which they live are scarcely scratched by them, and even in the great Ophiophagus, the snake-eating snake of India, whose natural diet consists of animals in which the circulation is so slow and vitality so sluggish as serpents that they are certainly swallowed before any poison could have time to work its effect upon them. In all probability the primary office of this remarkable fluid is to act as a digestive, it having been found by experiment that albumen, pieces of hard-boiled egg, etc., dissolve in this quite as readily as in the gastric juice of any flesh-eating animal. The writer has further established by his own experiments that small animals which have been sub-

mitted to the fangs of rattlesnakes, and other large viperine serpents are very much more quickly digested, not only by snakes, but by toads and other carnivorous reptiles and even mammalia, than pieces of meat or animals of corresponding species which have not been so treated.

There can be very little doubt that this morbific fluid, this venom, is a product of a recent evolution. The venom gland, although large, is distinctly one of the salivary glands in structure, one of the racemose group, very little altered in appearance from that which secretes the ordinary saliva, the venom being in fact an abundant saliva, and containing some toxic element the nature of which has not yet been distinctly ascertained, in addition to the ordinary salivary products. There is probably no other instance in nature of the enormous disproportion of change of function when compared with change of structure as obtains in the venomous fluid of the gland of a poison-bearing snake, unless indeed it be the function of the brain of man when compared with that of animals almost equal to him in complexity of cerebral structure.

There remains, however, a third group of serpents, gifted with the power of killing their prey before deglutition. These, which number possibly 400 or 500 species (the number not being accurately ascertained owing to absence of observation of living specimens), may be termed the Constrictive Group; and although no such physical distinction can be drawn between these and the ordinary or Colubrine snakes on structural grounds, as is at once apparent between the latter and the venomous group, yet the process of feeding is so entirely different, as to suggest the feasibility of establishing such a difference, by careful dissection. With these snakes the prey is slain at the moment of the seizure, by constriction, by being wrapped within the folds of the body and crushed to death; and this process is so remarkable in its vigor and in its rapidity, that it is impossible to imagine the creatures destitute of specially developed, if not specially supplied, muscles for this purpose.

This group includes not only the great Anaconda of tropical America, the very much smaller Boas of that region, as well as the Tree Boas, and the Pythonoid snakes of Africa and the East Indies, but very many smaller species as well. The black snake of North America is indeed distinctively named Coluber Constrictor; but there are very many other species manifesting this peculiarity which have as yet obtained no such distintive recognition, such as the Blue Racer of the States, the Saw-marked snake of South America, and the largest of the European serpents, the beautiful four-rayed Elaphis of Italy and Greece, which occasionally attains a length of six feet, and is capable of swallowing a large rat.

It is just possible that this power of constriction may have been acquired recently, like the venom of the poison-bearing snakes. Unfortunately, paleontology affords no evidence upon this point. We know very little of the evolution of the Ophidia. Fossils are very scarce; and although some of them, such as the noted specimen from the London Clay, suggest serpents of large size, and therefore presumably constrictors, we know nothing beyond what is suggested by mere inference as to whether they were gifted with venom, or had this property of constricting their prey before swallowing.

If we examine the lateral and intercostal muscles of one of the large Pythonoid snakes, we shall find that although these are very highly developed, and have indeed in certain instances small tendenous slips attaching them to the ribs, which are not found in smaller species, they are precisely analogous to the ordinary intercostal muscles which obtain through the whole of this family.

In certain species, such as the Milk snakes of the Northern states, and the Mandarin snake of China, we may occasionally see, when they are dealing with prey rather too strong for them, a sort of attempt made at constriction, a rapid coiling and uncoiling of the body, as though to confuse the animal struggling within the grasp of the jaws and teeth. And it is perhaps not wholly unjustifiable to imagine that this power of constriction may originally have been acquired in this way; that serpents which had previously fed, as our ordinary Colubrine snakes do, upon frogs, lizards, and soft-bodied animals which they could kill by pressure of the jaws alone, found themselves, for some reason or other, reduced to catching the smaller mammalia, mice, moles, etc., and that in their endeavors to get these within the cavity of the mouth, they found it necessary to bring the body into play to effect the purpose which had hitherto been accomplished by the jaws alone. One may, however, express the hope that when larger materials are at hand for examination, in the shape of the grander Pythonoid snakes, and most especially of the great Water Boa, the Anaconda of Central America, that some more definite information on this point will be gleaned.

NOTES AND NEWS.

At a recent meeting of the Canadian Institute, Mr. Andrew Elvins asked permission to add a sentence or two to his paper on the satellites of Jupiter, read at a former meeting. He said: "The period of each satellite as we pass outward from the planet is about double that of the one next inside itself, except in the case of Satellite I. Half its period would be about 21 hours, but there is no satellite having that period. Half of this 21-hour period is just where Professor Barnard's new satellite exists. Its period is between 11 and 12 hours. I therefore think that an undiscovered sixth satellite exists at 166,000 miles from Jupiter's centre, with a period of 21 hours."

— The faculty of the Museum of Comparative Zoölogy, Cambridge, Mass., will receive applications from candidates desiring to occupy the table at the Naples Zoölogical Station, which has been placed at its disposal from Oct. 1, 1893. The applicant must be (or have been recently) a student or instructor at some American university, preferably a person who has taken the degree of Ph.D. or S.D.; he must have published some creditable original investigation, and should be recommended as an able investigator by the professor under whom he has studied. Applicants will please forward to Professor Alexander Agassiz, Director of the Museum, before May 10, their recommendations and a statement of their qualifications and of the subject to which they hope to devote themselves. In order that the faculty may make the most satisfactory disposition of the table during the whole year, the applicants are requested to state the length of time they desire to remain at Naples, and also the earliest and latest dates within which they can avail themselves of the appointment. The faculty will, at its meeting in May, nominate to the Corporation of Harvard College for approval the incumbent or incumbents for the year 1893–94.

— The papers entered to be read at the April meeting of the National Academy of Sciences, are as follows: On the Systematic Relations of the Ophidia, E. D. Cope; Biographical Memoir of General Montgomery C. Meigs, H. L. Abbott; On the Nature of Certain Solutions, and on a New Means of Investigating Them, M. C. Lea; The Relations of Allied Branches of Biological Research to the Study of the Development of the Individual, and the Evolution of Groups, The Endosiphonoidea (Endoceras, etc.) Considered as a New Order of the Cephalopods, A New Type of Fossil Cephalopods, Results of Recent Researches upon Fossil Cephalopods of the Carboniferous, A. Hyatt; Biographical Memoir of Julius Erasmus Hilgard, E. W. Hilgard; Monograph of the Bombycine Moths of America, North of Mexico; Part I.—Notodontidæ, A. S. Packard; Intermediary Orbits, G. W. Hill; The Relations between the Statistics of Immigration and the Census Returns of the Foreign-born Population of the United States, Statistical Data for the Study of the Assimilation of Races and Nationalities in the United States, Richmond Mayo-Smith; Telegraphic Gravity Determinations, Comparison of Latitude Determinations at Waikiki, T. C. Mendenhall; A One-volt Standard Cell, H. S. Carhart (introduced by T. C. Mendenhall); Fundamental Standards of Length and Mass, T. C. Mendenhall; Peptonization in Gastric Digestion, R. H. Crittenden; Helen Kellar, Alexander Graham Bell; On a Potentiality of Internal Work in the Wind, On a Bolograph of the Infra-red Solar Spectrum, S. P. Langley; The Classification of the Gastropodous Mollusks, Theo. Gill. Presentation of the Draper Medal to Professor H. C. Vogel.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

OBSERVATIONS ON GOPHERS AND MOLES IN OREGON.

BY F. L. WASHBURN, STATE EXPERIMENT STATION, CORVALLIS, OREGON.

THE Zoölogical Department of the Oregon State Experiment Station has endeavored for two years to find some remedy for the pocket gopher and common mole found in this section. The first named, *Thomomys bulbivorus*, is a bad pest, and the decree has gone forth that our mole, *Scapanus townsendii*, is to be placed in the same category. Various traps purporting to catch gophers and moles are on the market, but few of them are reliable. Two, however, have been found to do very effective work in the case of both animals. Of poisons, powdered strychnine, introduced into pieces of potato an inch square and thrust down the burrows, has proved efficacious in the case of gophers in the absence of succulent root-crops. And small pieces of beef, poisoned with the same agent, have been placed in moles' burrows with occasionally good results, though nothing conclusive can be claimed for that now.

It is, however, of a few habits of the gopher, and more particularly of some interesting discoveries regarding the diet of the mole that we would here treat.

The pocket gopher has not a little intelligence. The horticulturist of this station reports finding a nest stored with potatoes, the tubers lying in layers, and each layer separated from the adjoining layers by more or less dried grass. The entrance to this nest, or at least one entrance, was from below, affording a perfect system of drainage quite desirable in this country of wet winters.

Again, it is a matter of frequent occurrence to see gopher mounds arranged in a straight line from that side of a field or garden upon which the gopher enters to some fruit-tree or potato, parsnip, or carrot patch, indicating that his main burrow beneath the surface has been pushed directly to these sources of food-supply. This main burrow, by the way, is from twelve to twenty inches below the surface, and has leading from it, at intervals, short branch burrows, which open on the surface of the ground and afford a means of getting rid of the soil excavated below. These branches are generally plugged with soil and their openings covered by a mound. The last one made, however, is often open, and the occupant of this underground retreat can frequently be seen protruding his head and disposing of the soil he has brought from the main burrow. As to the method of bringing out this soil opinions differ, some observers claiming that it is carried in the pockets, to some extent at least, and then thrown out of the pockets by movements of the fore-feet. Others deny this, asserting that the dirt is pushed before the animal and that the pockets are not used in this work. The writer has frequently secured specimens with forage in these pouches, but has never found them to contain earth, even in specimens killed in the act of excavating.

This rodent works during the night and is quite likely to be found digging early in the morning, again about noon, and again late in the afternoon. It is claimed that both the gopher and mole are more active, as regards digging, just before rain.

The nest of the pocket gopher is often found filled with camass bulbs, of which this animal is very fond, as much as a bushel of bulbs being reported as found in a single nest. When in the vicinity of gardens, however, more palatable food is found with which to store the larder.

The tender roots of young fruit-trees are, unfortunately, very tempting to these animals, and a dying cherry or apple can frequently be easily lifted from the ground, the root, gnawed completely through, showing the cause of its demise.

This leads to the subject of the mole's diet. Many, or most, of our scientists have united in defending the mole against charges of eating bulbs and other vegetable matter, and have stoutly asserted that the gnawed carrot, or parsnip, or crocus bulb, found in the course of the mole's burrow, was the work of one of the meadow mice. This is doubtless true. But that the mole occasionally, or possibly frequently, resorts to a vegetable diet must be acknowledged. A lady in Portland, Ore., quite sure that moles were eating her crocus bulbs, and feeling far from convinced of their innocence from the assertions of scientists, obtained three, which she kept in confinement. She found that they readily ate the following: Beef, mutton, pork, bread, wheat, pears, and peas. Unfortunately two of these pets were fed with worms taken from an old manure heap and died, showing symptoms of being poisoned. The description of these worms, as given me, answered to that of *Lumbricus fœtidus*, and it was undoubtedly that or an allied form which caused the trouble. Evidently this species of the Oligochetæ does not figure, naturally, on the mole's bill of fare. The writer witnessed the survivor eat peas greedily, running his sensitive snout from one end of the pod to the other and taking out every pea. This was convincing proof that the mole, under some circumstances at least, is not strictly carnivorous, and it is quite likely that he is frequently a malefactor as regards vegetables and roots. Personal examination of stomachs in specimens secured in March, 1892, revealed nothing but finely triturated earthworms, insects, and insect larvæ. In one captured in January, 1893, the stomach contained nothing but delicate, fibrous roots.

Amos W. Butler of Brookville, Ind., in speaking of moles, says: "I have never been satisfied that the mole in sandy soil is not very destructive to young pea sprouts just as they are emerging from the ground."

Both gophers and moles are active here during the winter season.

A word as to the breeding season may not be out of place. My diary states that on Feb. 28, 1892, a pregnant mole was captured containing three well-developed embryos, and two days later another was obtained with two embryos, apparently within a few days of birth. March 28, 1893, a pocket gopher was secured containing four young embryos. All this indicates an early date for the first litter. Probably more than one litter is produced. From specimens of Arvicolinæ secured it would appear that the breeding time of the field-mice is contemporaneous with that of the other two animals under discussion.

ONE OF THE GYPSUM CRYSTALS FROM THE CAVE AT SOUTH WASH, WAYNE CO., UTAH.

BY ALFRED J. MOSES, MINERALOGICAL LABORATORY, COLUMBIA COLLEGE, NEW YORK.

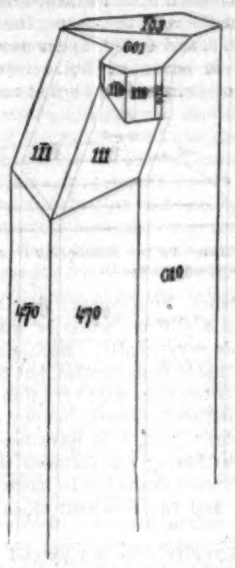
THE Deseret Museum of Salt Lake City sent last month to Columbia College two specimens from the remarkable deposit of Selenite, in Southern Utah, which was described in a recent issue of this paper.[1]

The larger of the two specimens was a portion of an evidently longer prism with very perfectly developed terminal planes. The specimen internally is colorless and glassy, but the surface is in part covered by a thin opaque layer. The weight of the specimen

[1] Feb. 17, 1893, pp. 85-86.

is 24½ pounds, its greatest length is 27 inches, its thickness (in direction of the ortho axis), is 4 inches, and its breadth (at right angles to the ortho axis) is six inches.

The angles of the crystal were taken with the hand goniometer. The most noticeable fact is that the unit prism of 111° 30′ occurs only on a curious prismatic extension, composed of this prism and a clino prism (470), which pierces the pyramidal plane 111 and extends upward about one inch to the basal plane common to this and the rest of the crystal as shown in the figure. The prism which occurs on the rest of the crystal has an angle of



approximately 79°, corresponding to the clino prism $i-\frac{7}{4}$ (470), and all its faces are striated vertically while those of the unit prism are smooth.

The other occurring forms are $i-\bar{i}$ (010), -1 (111), $\frac{1}{3}-\bar{i}$ ($\bar{1}$03), and O (001). The cleavages parallel to (010), ($\bar{1}$01) and ($\bar{1}$11) were visible in the break at the lower end.

At least six phantom terminations can be seen apparently parallel to (001) and ($\bar{1}$03).

DISTANCE AND COLOR PERCEPTION BY INFANTS.

BY J. MARK BALDWIN, PRINCETON, N. J.

I UNDERTOOK at the beginning of my child H's 9th month to experiment with her with a view to arriving at the exact state of her color perception, employing the new method which I described and compared with other methods in a recent paper in this journal.[1] The method consisted in this instance in giving the infant a comfortable sitting posture, kept constant by a band passing around her chest and fastened securely to the back of her chair. Her arms were left bare and quite free in their movements. Pieces of paper of different colors were exposed before her, at varying distances, front, right, and left. This was regulated by a frame-work, consisting of a horizontal graded (in inches) rod, projecting from the back of the chair at a level with her shoulder and parallel with her arm when extended straight forward, and carrying on it another rod, also graded in inches, at right-angles to the first. This second rod was thus a horizontal line directly in front of the child, parallel with a line connecting her two shoulders, and so equally distant for both hands. This second rod was made to slide upon the first, so as to be adjusted at any desirable distance from the child. On this second rod the colors, etc., were placed in succession, the object being to excite the child to reach for the color.

So far from being distasteful to the infant, I found that with pleasant suggestions thrown about the experiments, the whole procedure gave her the most intense gratification, and the affair became her most pleasant daily occupation. After each sitting she was given a reward of some kind.

The accompanying tables give the results, both for color and distance, of 217 experiments. Of these 111 were with five colors and 106 with ordinary newspaper (chosen as a relatively neutral object, which would have no color value and no association to the infant). In the tables *R* stands for "refusal" (to reach out for the object), *A* for "acceptance" (and effort), *N* for the entire number of experiments with each color respectively, and *n* for the entire number with all the colors at each distance respectively. So $\frac{A}{N}$ = the proportion of responses or efforts for any color, and $\frac{R}{n}$ = the proportion of refusals for each distance.

Table I.

Distance, inches.	9	10	11	12	13	14	15	Totals.	Ratio $\frac{A}{N}$
	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A. N.	
Blue.	0—1	0—4	0—5	1—8	2—4	1—5	3—1	7—23—30	.76⅔
Red.	0—1	0—3	2—2	1—4	1—7	1—7	5—1	10—25—35	.71⅓
White.	0—0	0—0	0—0	0—1	0—5	1—1	3—0	4—7—11	.63⅔
Green.	0—0	0—1	0—1	2—1	1—4	1—2	2—0	7—9—16	.56¼
Brown.	0—1	0—2	2—1	3—2	0—3	2—1	2—0	11—10—21	.47⅓
Totals.	0—8	0—10	4—9	7—11	4—23	7—16	15—2	37—74—111	.67
Ratio $\frac{R}{n}$	0	0	.33⅓	.39	.15	.30⅓	.89		

Table II.

Distance, inches.	9	10	11	12	13	14	15	Totals.	Ratio $\frac{A}{N}$
	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A. N.	
Newspaper.				0—17	0—28	1—33	25—2	26—80—106	.75⅓
Color.	0—8	0—10	4—9	7—11	4—23	7—16	15—2	37—74—111	.67
Totals.	0—8	0—10	4—9	7—28	4—51	8—49	40—4	63—154—217	.71
Ratio $\frac{R}{n}$			.30⅓	.20	.07⅓	.14	.91		

Color.—The results are evident in the tables (I. and II.), especially the columns marked "Ratio $\frac{A}{N}$" and "Ratio $\frac{R}{n}$." The colors range themselves in the order of attractiveness, i.e., blue, red, white, green, and brown. The difference between blue and red is very slight compared to that between any other two. This confirms Binet as against Preyer (who puts blue last), and also fails to confirm Preyer in putting brown before red and green. Brown to my child — as tested in this way — seemed to be about as neutral as could well be. White, on the other hand, was more attractive than green. I am sorry that my list does not include yellow. The newspaper was, at reaching distance (9 to 10 inches) and a little more (up to 14 inches), as attractive as the average of the colors, and even as much so as the red; but this is probably due to the fact that the newspaper experiments came after a good deal of practice in reaching after colors, and a more exact association between the stimulus and its distance. At 15 inches and over, accordingly, the newspaper was refused in more than 92 per cent of the cases, while blue was refused at that distance in only 75 per cent, and red in 84 per cent.

Distance.—In regard to the question of distance, the child persistently refused to reach for anything put 16 inches or more away from her. At 15 inches she refused 91 per cent of all the

[1] Science, April 21, 1893.

cases, 89 per cent of the color cases, and, as I have said, 92 per cent of the newspaper cases. At nearer distances we find the remarkable uniformity with which the *safe-distance* association works. At 14 inches only 14 per cent of all the cases were refused, and at 13 inches only about 8 per cent. The fact that there was a larger percentage of refusals at 11 and 12 inches than at 13 and 14 inches is seen from the table (I.) to be due to the influence of the brown, which was refused consistently when more than 10 inches away. The fact that there were no refusals to reach for anything exposed within reaching distance (10 inches) — other attractive objects being kept away — shows two things: (1) the very fine estimation visually of the distance represented by the arm-length, thus emphasizing the element of muscular sensation in the perception of distance generally; and (2) the great uniformity at this age of the phenomenon of "sensori-motor suggestion"[1] upon which this method of child-study is based.

In regard to the relative use of the two hands in these and other experiments, — this is a topic to which I wish to devote another paper, giving details upon which certain conclusions (announced in an earlier note in this journal) are based.[2]

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

The Convex Profile of Bad-Land Divides.

UNDER this caption Professor W. M. Davis, in *Science*, Oct. 28, 1892, discusses the "missing factor" in Gilbert's "Law of Divides," and concludes that it is "the creeping of the surface soil."

In my class-room lectures, and in a paper forwarded four months ago to the secretary of the Geological Society of America, but not yet published, I also have attempted an explanation of this missing factor. I mention this merely for the truth of history, not that I care much for the credit of priority, or fear the charge of plagiarism when my explanation appears. Its independent origin will be self-evident, because I have approached the problem in a very different way.

Both Professor Davis and Mr. Gilbert seem inclined to regard bad-land forms as something apart from land-sculpture in general — something which requires special explanation — while I have cited general laws and deduced these forms from them. My paper is entitled "Some Elements of Land-Sculpture: Water Curves, Weather Curves, and Structural Angles." Water curves are either horizontal, e.g., the serpentine course of a river, or vertical. The vertical water curve of erosion is concave upwards, e.g., the normal gradient of a stream excavating its channel in homogeneous material (b.c. Fig. 3); and the vertical water curve of deposition is convex upward, e.g., a *débris* fan, or alluvial cone.

All weather curves are convex upward. This fundamental law of the weather curve I have deduced theoretically in two ways, and that it is confirmed by observation almost goes without saying. An angular structural block, *A*, Fig. 1, is rounded by

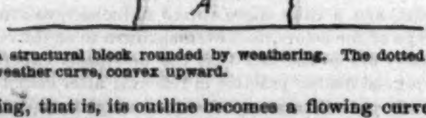


FIG. 1. — A structural block rounded by weathering. The dotted line is the weather curve, convex upward.

weathering, that is, its outline becomes a flowing curve, convex upward, like the dotted line in the figure, because the protruding angles are more exposed to attack, and at the same time the products of disintegration are in a position to be quickly removed. The complex forces included under the general term weathering have a double advantage at *a* as compared with *b*, because the attack comes from two directions. Moreover, the removal of loosened particles, whether by falling raindrops, by winds, or by gravitation (one effect of which is creeping), proceeds many times faster at *a* than at *b*. By a similar but slightly modified process of reasoning, it may be shown that a sharp crest triangular in cross-section would be rounded also by weathering (c.f. La Noé and Margarie, Les Formes du Terraines).

Another method of deducing the upward convexity of weather curves is that which is based upon the law of slopes in relation to hardness. The harder the rock the steeper the slope, other things being equal. Let 1, 2, 3, and 4 (Fig. 2) denote strata which grade regularly downward in hardness, No. 1 being hardest of all. Then, if the products of disintegration are at once and completely

FIG. 2. — Convex slope formed by the weathering of rocks which regularly increase in hardness downwards.

removed, as, for instance, by a stream flowing at *a*, the hard rock, No. 1, will form a cliff *ab*, while *bc* will be less steep, *cd* still less steep, and *de* very gentle. Each element of the slope, e.g., *bc*, is a straight line in cross-section, but the general effect is that of a curve; and if the beds were very thin it would pass from a broken line to a true flowing curve, convex upward. Now we may conceive the series 1, 2, 3, 4 to have been originally homogeneous, and that weathering has softened the upper members. In that case the downward gradation in hardness would be by infinitessimal laminæ, and the resultant slope a typical weather curve.

Ordinarily, the convexity does not extend to the bottom, because the weather curve is there replaced by the vertical water curve of erosion. This combination of weather and water curves modifying structural blocks yields the form shown in Fig. 3, the

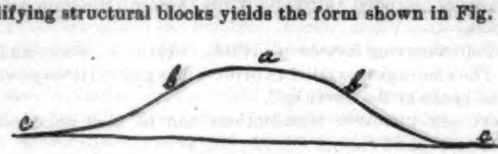


FIG. 3. — Cross-section of any ordinary ridge or hill.

most typical, as it is also the most familiar and universal of earth-forms. The upper part, *ab*, of each slope is a weather curve, convex upward, and the lower part, *bc*, is a water curve, concave upward. Bad-land divides are excellent examples of the general law, instead of being exceptions to it. The convex profile of the summit which puzzled Gilbert is simply the familiar and omnipresent weather curve. The only thing exceptional about it in the bad-lands is its narrowness and sharpness of curvature. That depends chiefly upon the early stage of the base-levelling in those regions, as I have shown in my forthcoming paper.

Creeping is a real factor in the rounding of divides, but is only one phase of the secondary process of transportation. Disintegration is the primary process. And in the subsequent movement of loosened particles, falling raindrops, gusts of rain driven aslant by winds, the winds themselves, the rolling and tumbling effects of gravitation as distinguished from the slow process of creeping — all these are active and efficient agents of removal. Their combined effects overshadow the results of creeping, especially on the bare, sharp ridges of the bad-lands. The clays are compact and firmly adherent. It is on gentle and turf-bound slopes that the slow process of creeping is relatively most effective.

Nor do I agree that the weather curve on the summit of bad-land ridges would be obliterated if the rainfall should increase. The effect of falling raindrops belongs to the category of weathering, and produces convex curves. It is only when the fallen drops gather into rills and begin to *flow* that the concave water curve of erosion begins to form. Hence increased rainfall would

[1] See my article on "Suggestion in Infancy," Science, xvii., 1891, p. 113; also my "Handbook of Psychology," Vol. II., pp. 297 ff.

[2] Science, xvi., 1890, p. 247.

probably strengthen rather than obliterate the weather curve, especially when we consider the effect of increasing vegetation which would follow increased rainfall. L. E. HICKS.

Lincoln, Neb., Nov. 4, 1892.

The Moon's Atmosphere.

IN *Science* of Feb. 24, Sir Robert Ball makes application of the kinetic theory of gases to explain the absence of air from the moon. He observes that, although the mean molecular velocity of translation is less than that required by a body projected vertically from the moon to overcome the moon's attraction, "in the course of their movements, individual molecules frequently attain velocities very much in excess of the average pace," and would therefore be able to escape from the moon into space, and thus, in time, the whole atmosphere would be lost. I think a full consideration of the subject will not justify that conclusion, but that we shall be obliged to resort to some other physical laws to solve this old problem of speculation.

The kinetic theory requires all the molecules of a gas to have equal masses, equal energies, and hence equal mean velocities. This mean velocity for the hydrogen molecules at 0° C. is about 1,800 metres per second, while that of oxygen and nitrogen is about 450 metres per second, since the velocity is inversely proportional to the square root of the mass of the molecule. To overcome the moon's attraction a body must have a vertical velocity of about 2,200 metres per second. But it must be remarked that the escaping molecules, if there are such, are only those of the outer confines of the atmospheric envelope, where the mean free path of the molecules is relatively very great, as suggested with respect to the earth's atmosphere by H. Daniells ("Principles of Physics"), and the temperature of those regions is very low. If the temperature is about 68° absolute scale (−204° C.), as assumed by some authorities, the mean molecular velocity falls to about 225 metres a second, since the velocity varies as the square root of the absolute temperature. The vertical velocity, then, or the vertical component of the velocity must be about ten times the mean velocity to balance the force of gravitation, which is not probable.

Again, if the temperature is much lower than 68° absolute, approximating the absolute zero, and the molecular velocity always obeys the law before mentioned, the velocity also would approximate zero, and of course the molecules could not escape the attraction. It appears, then, to be largely a question of the temperature of the outer limits of an atmosphere. With this in view, let us compare results on planetary bodies of different size and stage of world life. As already suggested, with respect to the earth and moon, the earth's attraction at the surface is about five times that of the moon at its surface. This, *cæteris paribus*, would require about five times greater molecular velocity of its atmosphere to escape than for that of the moon. But, if we take into account the previous history of the two bodies, it is observed that the earth was highly heated for ages after the moon had become comparatively cool, and this must have rarefied and expelled its atmosphere to great heights, and maintained a temperature in those regions which, according to the proposition under discussion, would have caused the earth to lose its atmosphere. In general, it would follow that the major planets and larger satellites would lose their atmospheres more completely while cooling than the smaller ones, unless they have correspondingly greater quantities of volatile matter in their composition than the smaller ones. And such seems to be the result. Even Jupiter, whose attraction at the surface is 2.6 times that of the earth, is believed to have an atmosphere much less extensive proportionately than the earth. Mars offers a good example of a small planet with a copious atmosphere. Its attraction is only about twice that of the moon. Why has he not lost his atmosphere? If the application of the kinetic theory alone explains the loss of the moon's atmosphere, it would require Mars to have suffered the same fate before now. Possibly we are committing the error of the Greek philosophers in treating molecules as independent masses instead of regarding them as inter-dependent centres of activity whose phenomena, as a system, constitute the qualities of matter. I do not assume to offer a solution for this complex problem, but hope rather to encourage discussion which will call out all the principles of physical science applicable to it. W. H. HOWARD.

Adrian College, Adrian, Mich., April 15.

Note on the Crystalline Lens of the Eye.

MR. MCLOUTH'S observation upon "A Peculiar Eye," as observed by him in "a domestic animal," given in *Science*, No. 531, would have been considerably enhanced in value had he recorded at the same time what that "domestic animal" was; whether it was an anserine fowl, as a duck or goose; or a gallinaceous one, as a hen, turkey, peacock, or guinea-fowl; or whether a carnivorous mammal, as a dog, or a cat; or an *Equus*, or a *Bos*, or a *Sus*, or an *Ovis*, or what not.

To the minds of some, the so-called "domestic animals" form a natural group, and even such an authority as Girard was so blind as once to propose a *special* classification for the domesticated mammals! It is not uninteresting to trace the origin of this idea, associated as it is in a way with the kindred one of man holding a place apart from the rest of organized beings.

It is only necessary to invite Mr. McLouth's attention here to the fact that the crystalline lens in the eye of man consists of *three* triangular segments, and their existence is easily demonstrated by immersion of the lens in strong alcohol, or by boiling it. The apices of these three segments are at the centre of the lens, in *front;* their bases in the circumference. Another structural feature of the lens is seen in the laminæ of which it is composed. The treatment just proposed demonstrates these also, consisting, as they do, of concentric layers, which are firm at the centre, but become softer as we approach the peripheral ones. Likewise, by thus treating the crystalline lens from the eye of a horse, we prove that it also divides into its concentric laminæ, and its *three* triangular segments. But whether this holds true in the case of all vertebrates has not, I think, been demonstrated. Very likely the crystalline lens of the "domestic animal" examined by Mr. McLouth had been submitted to a process which had a similar effect upon it as boiling or immersion in alcohol would have had, and simply exhibited its normal structure. From what I can gather from the communication of your correspondent in *Science* there was nothing abnormal about the lens of the eye he examined.

R. W. SHUFELDT.

Takoma, D.C., April 14.

The Aurora.

IN *Science* for April 7, at page 186, certain statements of mine in regard to auroral effects proceeding from the sun's eastern limb are called in question. It would have been much more satisfactory if these criticisms had given evidence of such familiarity with the subject as would be shown by the mention of even a single date on which it might be claimed that an aurora appeared in the absence of well-defined solar conditions of the character indicated. Except where specific mention is made of such individual instances, the writer proposes to refrain from discussion, which would readily become interminable as well as utterly inconclusive. Such results as those of Professor Ricco, recently announced in *Astronomy and Astro-Physics* and elsewhere, it is a pleasure to meet with and comment upon. He simply takes the case of the great magnetic storms of 1892, which were eleven in number, and studies the coincident solar conditions, especially with reference to the location of spot groups at the meridian. In seven out of the eleven instances he finds that there were such groups on the meridian, but that the magnetic effect, if it proceeded from them at all, was not felt for a varying period of from twenty-one to fifty-one hours subsequently. If, however, he had gone further and inquired what there was at the eastern limb on these dates, he would have found that there was a spot group in that location in every one of these instances without any exception whatever, and that these groups were located upon areas which were much disturbed at successive returns by rotation. Moreover, there was in these instances no appreciable retardation or variability of retardation, the magnetic storm being in progress

on the very dates when the disturbed sections were in process of being brought into view by rotation. Perhaps the most striking illustration of the whole matter in a single instance is to be found in the history of a great disturbance upon the sun in January, 1886. Upon the 12th of that month spots suddenly began to form almost precisely at the meridian and about 10° south of the sun's equator. Upon the four days following, these spots became numerous, and some of them very large, covering an enormous area, extending finally from the meridian almost half-way to the western limb. It would seem that if magnetic effects ever proceed from the sun's meridian that this, above every other, should have been a case in point. But there was scarcely any disturbance whatever and no auroras were reported from any source. On Jan. 16 and 17 the magnets were entirely free from disturbance when this great spot-group was undergoing many rapid changes and was generally in the precise location to have a terrestrial magnetic effect according to the idea which Professor Ricco attempted to work out as above described. When, however, this area was at the eastern limb, from Jan. 7 to 11, although it had not yet developed spots and was the seat of groups of brilliant faculæ only, there was an entirely different state of affairs, a great magnetic storm being in progress and auroras being reported generally from localities in high latitudes. Thus it appears that it is not faculæ in general that produce such marked effects, but faculæ in the location of areas frequented more or less persistently by spots, etc.

M. A. VEEDER.

Lyons, N.Y., April 14.

Where is the Litre?

I HAVE read Professor Mendenhall's contribution to *Science* of April 21 with surprise. I did not think it possible for so eminent a man to so entirely miss the point of any article he might condescend to read and criticise. Nor did I think it possible for so keen-witted a controversialist to so entirely forget his own argument as to admit and corroborate the very statements he set out to refute. Yet any reader of *Science* who may take the trouble to read the two articles written respectively by Professor Mendenhall and myself under the heading "Where is the Litre?" will see that both of the unlikely events in question have happened.

I invite my distinguished critic to re-peruse the paper he attacks, and to thus ascertain whether it contains any statements or contentions displaying "ignorance of the recognized principles of metrology," or whether it sets forth "certain conclusions which will generally be harmless on account of the very magnitude of their errors." If he can find any statements, contensions, or conclusions that appear to him to justify such descriptions, let him quote them in their *ipsissima verba*, and let him show in what manner they betray ignorance or error. I will then, in my turn, show the Professor to be mistaken.

This is no over-bold challenge. It is almost self-evident that Professor Mendenhall was unable to find any display of ignorance or any erroneous conclusion in my article; as, in that case, he would naturally have quoted the offending passages in justification of his severe remarks. But his only approach to quotation is worded as follows: "The sermonizing finish to the article, beginning with the sentence, 'In spite of the much lauded simplicity of metric measures,' etc., may, however, mislead a few readers whose ideas have been befogged by the perusal of the previous three pages." Such a reference is too loose, too indefinite, and too general to indicate what particular statements or conclusions are objected to; and the Professor's scornful allusion to easily-befogged readers of *Science* is, perhaps, too donnish.

And now, while leaving my critic to the digestion of my challenge, I may, without impropriety, quote some opinions that have reached me from other authorities.

1. The *Engineering News* of March 30, in an editorial reference to my paper, says: "Different enactments by legislative bodies, errors in measurement and in calculation, difference in weights between bodies weighed in air and weighed in vacuo, and difference in weights between water containing air and water freed from it have conspired to produce these variations. It is true these variations are all so small as not to affect the practical accuracy of any ordinary measurements; but for the exact work of physicists and chemists, and for some of the finer measurements of engineers, these variations are sufficient to affect the results. The moral which Mr. Emmens points is that the author of any paper or treatise claiming scientific accuracy, and dealing in quantities whose exact values may be in doubt, should preface his work with a statement of the constants adopted throughout the work. In a personal letter to us Mr. Emmens makes the further suggestion that the international congress of scientists and engineers at Chicago next summer will afford an excellent opportunity for defining anew the metric standards whose values have become most variable, thus restoring to the system the advantages of simplicity and freedom from ambiguity which it was originally intended to possess. It certainly gives good ground for criticism that in every school in the land pupils are taught that the litre is equal to the cubic decimetre, whereas, in reality, the litre is about 0.1 cubic inches larger than a cubic decimetre, the exact variation depending on what value is chosen for each."

2. Professor De Volson Wood, of the Stevens Institute, writes: "Your article in *Science*, 'Where is the Litre?' is such a model of courteous discussion that I thank you for it. The closing remarks contain sentiments I often advocate, but you have done it so much more completely and in all respects so much better than I could, that I appreciate it."

3. Mr. R. A. Hadfield, of the Hecla Steel Works, Sheffield, England, whose scientific reputation is world-wide, writes: "It appears to me you have touched the weak point of the Metric system, and it was only the other evening, at a lecture on this subject, that I was aware for the first time there was a difference between the litre and the cubic decimetre. No doubt many others are in the same way, and it would therefore be specially desirable to have some common understanding on this matter."

4. Mr. Latimer Clark, F.R.S., writes: "I will see the Board of Trade with your letters. They are as anxious as you or I can be to help in such a cause, and would do anything to promote it. The Chicago conference would afford a capital opportunity for raising the question, and I will do anything required if you will point out what you recommend. The difference between the litre and cubic decimetre is simply one of popular belief and teaching, and it arises from the French Bureau having decided to adopt the bulk of the kilogramme of water as the bulk of the litre. I may perhaps add that the Warden of the Standards here has written me that he acknowledges my dictionary as correctly setting forth the values they have adopted and are employing, and he adds that he recommends the book to all enquirers on the subject."

I refrain from adducing further evidence lest I should put Professor Mendenhall in the position of the dissentient juryman who complained that "he had never before, in the whole of his life, met with eleven such obstinate fellows."

STEPHEN H. EMMENS.

Youngwood, Pa., April, 25.

Sham Biology in America.

MR. CONWAY MACMILLAN has shown more enthusiasm than discretion in his recent article. He is writing in a good cause, namely, the elevation of botany to an equal rank with zoölogy in biological teaching in universities. Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life; and I am not aware that biology is taught in any large institution in this country without taking advantage of the fact that certain laws and principles of life are, for purposes of practical study, far better shown in plants than in animals. Plant biology is therefore extensively taught upon the lines laid down by Huxley and Martin, and on such lines we simply select the organism which best demonstrates a certain principle. If the botanists of this country allow the zoölogists to take the lead as *biologists*, that is, in setting forth the fundamental principles of life from their observations upon animals, it will naturally follow that zoölogy will occupy the leading position in the universities. Mr. MacMillan's argument should therefore be directed to the botanists and not to the zoölogists, who are in no

way responsible for the alleged one-sided state of biological education.

While Mr. MacMillan's enthusiasm is in a good cause, he has allowed it to run away with his discretion. Without sufficient reflection or inquiry, he has, unintentionally, I am sure, given an entirely wrong impression of the character of work done in several institutions; this is done under a very sensational title and in a style of questionable taste. As it is desirable that this impression should not spread, and as the arrangement of courses in Columbia is cited by Mr. MacMillan as a leading example of the manner in which botany is subordinated to zoölogy, let us see what the Columbia courses are, as announced in the circular of the faculty of pure science:—

17 Courses in Botany, in A. Department of Botany.	1. Elementary Botany. 2. Elementary Botany. 3. General Botany. 4. Vegetable Anatomy. Cells and Tissues. 5. Morphology and Determination of Flowering Plants. 6. Economic Botany. 7. Cryptogamic Botany. 8. Advanced Vegetable Anatomy. 9. Natural Orders of Flowering Plants. 10. Advanced Cryptogamic Botany. 11. Comparative Study of Tissue of Twelve Species. 12. Comparative Study of Plants from a Certain Area. 13. Critical Study of a Genus.
in B. Department of Geology.	1. Palæobotany. 2. Study of Flora of Certain Geological Horizons.
3 Courses in Physiology, in Department of Physiology.	1. General Physiology. Lower Animal Types. 2. Human Physiology. Man and Lower Animals. 3. Laboratory Physiology.

There are altogether eleven courses in zoölogy under the Department of Biology, two of which, namely, "Elementary Biology" and "Cellular Biology" are taught in part from plants.

It does not appear that botany is ignored in this programme of biological courses of study in this institution. The fact that the botanical courses are not arranged under the Biological Department is a mere technicality of administration, which raises no confusion in the minds of students, any more than does the separation of the Department of Physiology, which is equally cognate to biology. The separation of these three departments is simply owing to the fact that botany and physiology were already well established when the trustees decided to found a distinct department in which biology would be taught especially as illustrated in animal types.

HENRY F. OSBORN.

Columbia College, New York, April 18.

Cedar Waxwings.

Mr. Edwin M. Hasbrouck's "Presumably new fact relative to the Cedar Waxwings (Amp. Ced.)" in the issue of the 17th ult., is a very interesting discovery. The observations from which his conclusions were obtained, are familiar to modern ornithology, while his inductions are assuredly new to me. Whether they are accepted or not, his views of the importance of carefully studying the *first* plumages of birds will scarcely fail of universal acceptance. I have no criticisms, but wish to add an observation concerning the wax tips of the secondaries and retrices of the species which I am inclined to think will favor his conclusions.

I have made the ultimate anatomical structure of feathers a special study for many years, during which I have given those of the period before the first moulting special consideration, and have met with some extremely interesting things.

I have never been so fortunate as to meet with a wax tip while the young bird was still in the nest, but have occasionally seen them in very fresh subjects, or as early as the 25th of July. The development of the appendage, after it has commenced to appear, is very rapid indeed, resembling the process of the growth of the new antlers of a buck. I cannot yet state definitely the length of time, but from three to five days ordinarily, and doubtless sometimes a little more. In a work devoted to the Birds of Minnesota, I have made some references to my familiarity with the species, to which I might add many more notes, since that went out of my hands, that are even more in point, but suffice to say, the red wax is secreted in the ciliohamular portion of the barbules of the terminal barbs of the feather.

The rapidity of the development of the appendage is such that occasionally it results in doubling the whole series of barbs with their barbules, back upon the rachis of the feather, and reveals the fact that the horny material constituting the wax-like mass is filled from the tip, shaftward, as if in fact, as in appearance, it consists of genuine red sealing-wax, which has become so thickened or condensed as to cease flowing before quite reaching the point of union of the barb with the delicate, overladen rachis. The naked portion of those barbs becomes an easy object of observation under low powers of the microscope, and under supremely good light and a higher magnification, the reflected portions of the barb with its barbules, and even the barbicels, may be seen resting upon the unreflected portion of the barbs and rachis. That there is some special condition very temporarily involved, that produces these decorations, there can be no doubt. I have never yet succeeded in seeing a wax-tip on a waxwing reared in captivity, excellent as has been my opportunity. Who next has something new about the Cedar Waxwing?

P. L. HATCH.

An Appeal to Naturalists.

MAY I appeal through your valued columns for the coöperation of the naturalists of the country? The following letter from Professor Kölliker of Würzburg is the occasion of my appeal:—

WÜRZBURG, April 4, 1893.

MY DEAR PROFESSOR MINOT:

May I ask you if you could procure for me some rare American forms of fishes and amphibians, preserved in Müller's fluid, so as to be investigated microscopically after Golgi's and Weigert's method? Larger animals should be cut transversely, so that the fluid can enter the spinal canal and act upon the spinal marrow. At the same time the head or body should be opened and the brain acted upon.

The list of my wishes is very large, but I shall be very glad, if I get only some of the animals mentioned. It includes, among the amphibia and reptiles, Amphiuma, Siren, Menobranchus, Menopoma, full-grown and larval, young alligators and tortoises; among the fishes, Lepidosteus, Amia, Spatularia, Scaphyrhynchus, full-grown and also very young. . . . I am working just now at the microscopic anatomy of the nervous system, and have begun to extend my investigations to the comparative part also. Unfortunately, specimens in spirit only are worth very little, and the only good methods are those of Golgi and Weigert. But even Golgi's is only useful on embryos and young animals, and you know that both these methods demand a previous preservation in Müller's fluid.

Believe me, etc.,

A. KÖLLIKER.

In view of Professor Kölliker's distinguished services to science, covering a period of over fifty years, and of his undiminished activity in research, every one must feel a wish to promote any investigation Professor Kölliker undertakes. In order to secure the material for which Professor Kölliker asks, I seek for contributions from my American colleagues. I request that all specimens may be sent to me at the Harvard Medical School, so as to be in my hands by May 30. All material thus obtained can be packed and forwarded to Professor Kölliker, together with the list of contributors.

The specimens should be kept in the Müller's fluid until they reach Würzburg. In order to secure a good result with the fluid, it must be used in large quantities, and should be changed every day for the first week, and twice during the second week. Müller's fluid will not penetrate hard tissues, such as bone, for more than a quarter of an inch, and soft tissues for more than three-

fourths of an inch. On this account the pieces to be preserved must not be too large.

The best formula for Müller's fluid known to me is:—

Bichromate of potassium	2 per cent
Sulphate of sodium	2 " "
Water	96 " "

In practice it is convenient and sufficiently exact to dissolve two grammes of each salt in 1,000 cubic centimetres of water.

It will be, I am sure, a pleasure to many naturalists in America to learn of an opportunity of rendering a service to Professor Kölliker, to whom we all owe so much, and whose continued activity is perhaps the most remarkable instance of prolonged and fully sustained mental power in the whole history of biological science. We must all feel confident that any material placed at his disposal will be the means of securing important additions to knowledge.

CHARLES SEDGWICK MINOT.
Harvard Medical School, Boston, Mass.

BOOK-REVIEWS.

The Meaning and the Method of Life: A Search for Religion in Biology. By GEORGE M. GOULD. New York, G. P. Putnam's Sons. $1.75.

THIS book is a result of the unsettled and transitional state of religious opinion. Feeling deeply the want of some religion, but dissatisfied with the religions of the past, Dr. Gould has sought in the phenomena revealed by his favorite science of biology the basis of a new theology and a new religion. His views are somewhat singular. He holds that matter is eternal and independent of God, who is the author of life and mind only, using matter as the material of the living bodies that he forms, but having otherwise no control over it. Hence God is a limited and conditioned being, and, though very wise and perfectly good, is very far from omnipotent. This theory is somewhat like one that had some prevalence in ancient times, which also regarded matter as eternal and the Creator as merely the workman who fashioned it; yet the doctrine of this book limits Him still more, since it confines Him strictly to the world of life, excluding Him entirely from the vastly larger field of inorganic matter. In this way Dr. Gould thinks that he accounts for the existence of evil, which is due to the limited power of the Creator, whose goodness is thus saved at the expense of his omnipotence. Every living thing is an incarnation of Divinity, and man especially so. Man's duty consists in promoting the growth and fulness of life everywhere, and especially the spiritualization of human life. On the question of immortality, Dr. Gould expresses no decided opinion, holding that God has not seen fit to reveal his design with regard to man's future, and believing that information concerning it would be of no use to us here if we had it.

Such is Dr. Gould's religion; but, though it may find some favor among other biologists, we doubt if it wins acceptance anywhere, for religions and philosophies that deny the Divine omnipotence have never proved congenial to the human mind, and never will. His theory of the universe and its Author is evidently due to a too exclusive study of one science to the neglect of other and wider views, a mode of investigation peculiarly dangerous in theology. But whatever may be thought of his positive doctrines, all true souls will symphathize with the sentiment expressed in his introductory chapter, that "the bravest, noblest attitude is that of unsatisfied longing, and the never stilled faith that light will come into all of our darkness, and that the riddle of our lives will be solved."

Beiträge zur Kenntniss der Baues und Lebens der Flechten. II. Die Syntrophie. VON DR. ARTHES MINKS. WIEN, 1893.

DR. MINKS of Stettin, Prussia, is, or should be, known to all who are interested in the Lichens, and the controversy with regard to them, as one of the strongest advocates of their autonomy, on grounds peculiarly his own. In various publications he has announced the result of arduous and long-continued investigations, which are at least worthy of serious consideration. They cannot be ignored, as is the fashion among those who adopt the ideas of the new school.

CALENDAR OF SOCIETIES.

Biological Society, Washington.

Apr. 22.—O. F. Cook, Notes on the Natural History of Liberia; J. N. Rose, Two New Trees of Ec nomic Importance from Mexico; V. A. Moore, Observations on the Distribution and Specific Characters of the Streptococci Group of Bacteria; Erwin F. Smith, Peach Yellows and Plant Nutrition.

Geological Society, Washington.

Apr. 26.—The first half hour will be devoted to continuing the discussion concerning the Age of the Earth. Bailey Willis, Interpretation of Sedimentary Rocks; M. R. Campbell, The Influence of Post-Paleozoic Deformation on the Drainage of the Central Appalachians.

Academy of Sciences, Biological Section, New York.

Apr. 10.—H. F. Osborn, on "The Evolution of Teeth in Mammalia in Its Bearing upon the Problem of Phylogeny," reviewed the recent researches and theories of Kükenthal, Röse, and Tacker upon the formation and succession of the dental series in mammamalia, and pointed out that, especially in marsupials, cetaceans, and edentates (with other placentates), the existence of two series of teeth was now abundantly proven, as well as the fact that Homodynamous forms were derived from early Heterodont. He then showed that recent discoveries demonstrated that in marsupials teeth of the second series might be interposed in the first series—to explain the typical dentition of such forms of Didelphys. This transposition enables a comparison of dentition of marsupial with that of turassic mammalia $\left(= i, \frac{4}{4}, c, \frac{1}{1}, p, \frac{4}{4}, m, \frac{8}{8}\right)$. It was further noted that the triconodont type (as Amphilestes) was probably the hypothetical point of divergence of placental mammalia. As to the form of crowns, the theory (Kükenthal-Röse) that complex mammalian types were made by concrescence of simple reptilian cusps was upon the evidence of the turassic mammalia shown untenable, as well as the converse theory that cetaceans have derived homodynamous form by the splitting of the cusps of triconodont. Bashford Dean, in "Contributions to the Anatomy of Dinichthys," correlated the parts of this Devon-Lower Carboniferous Arthrodiran to those of Coccosteus. Notes were made upon the (1) disposition and character of the lateral line organs, (2) pineal foramen, (3) nasal capsules, (4) dentary plates (homologies), (5) ginglymoid articulation of lateral shoulder plates, (6) character of shagreen, (7) probable disposition of paired and unpaired fins. N. L. Britton presented a "Note on the Genus Lechea." This genus of Cistineæ is entirely American, and, from the investigations of Mr. Wm. H. Leggett and Dr. Britton, appears to consist of about fourteen species.

Agassiz Scientific Society, Corvallis, Ore.

Apr. 13.—C. D. Thompson, Relation of Soils to Plant Growth.

Mar. 8.—Professor John M. Bloss, The Early Lives of Some of Our Scientists.

In the work now before us, Dr. Minks considers the question of the so-called parasitic Lichens. In 1880, in "Morphologisch-lichenographische Studien," II., he had said that "Lichen and parasite are two irreconcilable conceptions." In the present work he develops this idea and extends it to a considerable number of Lichens, the apothecia of which had been previously considered to belong to the thallus on which they are found, and applies the term "syntrophy" to all such. In a syntropic lichen, by careful microscopic investigation, the epiphytic apothecium is found to have a very delicate thalline tissue of its own, from which it derives sustenance independently of the foreign thallus on which it grows. Dr. Minks describes this relation in language almost as picturesque as that used by Schwendener, in a phrase which has become classic. The syntrophic apothecia, he says, "are guests, because they offer nothing to the host, but claim services from him without compensation. But they are not boarders, else they might properly be termed parasites, but only lodgers. They are tenants, who pay no rent, but share the lot of the landlord."

In application of this doctrine, Dr. Minks considers a number of genera and species of Lichens, which, from his point of view, are syntrophic. Prominent among these is the genus Pyxine, the apothecia of which are syntropic on species of Physicia. Others are the Caliciacei and the Gyalectacei, the latter being elevated to the dignity of a tribe, while the author follows Nylander in combining the Lecanorei and the Lecideei in one tribe, the Lecano-Lecideei.

It were much to be desired that some of our younger botanists and microscopists, instead of consuming time in tedious and often superficial attempts to determine species, and of accepting as a dogma the Schwendener theory, neglecting to study what has been said on both sides of the controversy, would make themselves familiar with the copious literature of the last few years, and apply themselves to the study of the morphology and physiology of the Lichens, which, from whatever point of view they are considered, are among the most remarkable products of the vegetable kingdom. They might be able, by patient labor and by not being in too much haste to arrive at conclusions, to make valuable contributions to the vexed controversy. W.

The Story of the Atlantic Telegraph. By HENRY M. FIELD. New York, Chas. Scribner's Sons.

THE story of difficulties overcome in the endeavor to accomplish a great work is always interesting, and the account here given of the laying of the Atlantic cable reads like a romance. As is natural, since the writer is a brother of his, the work of Mr. Cyrus W. Field is given the most prominence, but we notice with pleasure that the indomitable perseverance and courage of the financiers engaged in the undertaking is recognized as it should be.

The Voltaic Cell. By PARK BENJAMIN, LL.B., Ph.D. New York, John Wiley & Sons.

DR. PARK BENJAMIN has had long experience in collecting the material for encyclopedic treatises, and the reader of this book may be sure that nearly all that could be collected on the subject has been incorporated here. This, however, does not necessarily constitute a good book, and in the present case the material used in cementing together the vast quantity of contained information might have been improved upon had a little more care been spent on this part of the work. For instance, while the list of cuts of storage cells is a very complete one, the theory of the changes which go on during the charge and discharge is hardly touched upon. The book contains a large number of valuable tables of conductivities of solutions, heats of combination, etc.

R. A. F.

THE Egypt Exploration Fund's new circular respecting the archæological survey of Egypt may be obtained from Dr. W. C. Winslow, 525 Beacon Street Boston.

THE American Bell Telephone COMPANY.

95 MILK ST., BOSTON, MASS.

This Company owns the Letters-Patent No. 186,787, granted to Alexander Graham Bell, January 30th, 1877, the scope of which has been defined by the Supreme Court of the United States in the following terms:

"The patent itself is for the mechanical structure of an electric telephone to be used to produce the electrical action on which the first patent rests. The third claim is for the use in such instruments of a diaphragm, made of a plate of iron or steel, or other material capable of inductive action; the fifth, of a permanent magnet constructed as described with a coil upon the end or ends nearest the plate; the sixth, of a sounding box as described; the seventh, of a speaking or hearing tube as described for conveying the sounds; and the eighth, of a permanent magnet and plate combined. The claim is not for these several things in and of themselves, but for an electric telephone in the construction of which these things or any of them are used."

This Company also owns Letters-Patent No. 463,569, granted to Emile Berliner, November 17, 1891, for a combined Telegraph and Telephone, and controls Letters-Patent No. 474,231, granted to Thomas A. Edison, May 3, 1892, for a Speaking Telegraph, which cover fundamental inventions and embrace all forms of microphone transmitters and of carbon telephones.

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